Thirty years of mesons

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Thirty Years of Mesons

Mesons have been predicted (in the first place, pi mesons by Yukawa in 1935),misidentified (mu mesons in cosmic rays) and discovered (pi mesons in cosmic rays), or at times discovered and not understood (the K mesons). Although the discovery of more mesons is expected, it seems unlikely that we are about to return to a traditional view of the fundamental particles as "simply" composed.

by Robert Oppenheimer

LET ME OPEN this account by reminding you that by now many mesons have been recognized, ordered and sorted out, their properties in part established, in some limited measure understood.

Thus there are three groups of nonets (some of you may with reason prefer to say mixed octets and singlets): the pseudoscalar mesons, which include the two that Yukawa predicted thirty years ago, the vector mesons, and the mesons with spin two, perhaps slightly less certain; and there are others now being identified. It would be a hazardous guess that we were now at the beginning or the end of this story.

Three families of particles

The mesons form one of the three families of particles of which, in addition to the quanta of the classical fields, we find it helpful and necessary to speak. They are characterized, these three families, either by the existence or irrelevance of quantum numbers satisfying ununderstood but very rigorous conservation laws.

There are the leptons, electron-like objects: the electron, the mu meson, the neutrinos and their antiparticles. The lepton number, the number of leptons minus the number of antileptons, does not change; this is a strict conservation law good even for cosmological times. The leptons have electromagnetic interactions, and weak interactions related to the Fermi interactions of beta decay. Under hitherto accessible observational conditions, these interactions are never very strong. The leptons have no direct or specific involvement with the strong interactions that characterize the other families of particles, mesons and baryons.

The baryons are proton-like objects: the proton, neutron, the Y*'s, the 3-3 resonance discovered by Fermi and his colleagues, the omega minus. For them, which also have their antiparticles, and which are characterized by a quantum number—baryon number—defined in analogy to lepton number, the baryon number does not change, here again even in cosmic times.

Mesons have neither of these quantum numbers: they can appear and disappear singly, subject, of course, to the conservation laws of relativity, to the symmetry between identical particles, to

PHOTO BY STELTZER

Robert Oppenheimer recently retired as director of the Institute for Advanced Study, a position he has held since 1947. He was born in New York City in 1904, received his bachelor's degree from Harvard University, and his doctorate from Göttingen University. From 1929 to 1947 he was professor of physics at the University of California in Berkeley and at the California Institute of Technology. This article is adapted from a talk delivered at the January meeting of the American Physical Society, in New York.

charge symmetry (although this and the next four do not provide strict or exact rules) to parity and strangeness conservation, charge-conjugation and time-reversal invariance, but nothing like baryon or lepton numbers.

Compelling arguments

Today our situation is, of course, very different than in 1935, but there are still analogies. In that year Yukawa predicted subnuclear objects that had not been observed. My principal purpose is to remind you of the nature of and reasons for what he did, and to follow some of the remarkable episodes that mark the three decades that separate us from those beginnings. Today we also have on the books undiscovered objects that have been sought experimentally, and for which, typically, the experimental statement is that they do not exist with a mass less than some rather high value. I have here less in mind the magnetic poles, which seem to me in a quite other category of speculation, than two other sets of objects, sometimes called, in disparate flashes of erudition, schizons and quarks. The former, studied and so named by Lee and Yang, which are vector bosons of undefined parity, strangeness and isotopic spin, were already foreshadowed by Yukawa: His mesons were to be beta-unstable, and by Yukawa coupling with baryon and lepton currents, were to induce beta decay. Until now the schizons have not been found; if they exist, they are substantially more massive than the proton. As higher neutrino energies become available, the search for them will doubtless continue. The quarks, in their simplest, most striking form, have fractional charge and baryon number. They too have not been found.

In all these cases—Yukawa mesons, schizons, quarks—the imagined existence of these particles makes it for a time somewhat easier to describe important regularities that we do observe among particles known to exist: nucleons for Yukawa's mesons thirty years ago; baryons, mesons, leptons today. If we were now confronted with the existence of the conjectured particles, we should face the most formidable theoretical problems of their description, more formidable no doubt for quarks than schizons, but for both wholly beyond what we know how to do. We could not explain dynamically how the new objects contribute to the regularities they were invented to account for. Indeed that happened for many long years with meson theory, and has taken many long years to bring to a partial, approximate and provisional resolution.

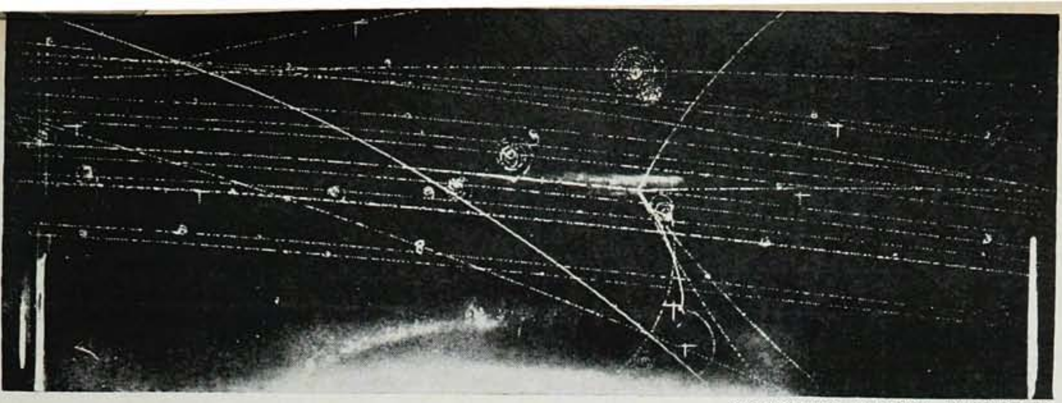
The reasons that have led to the invention of quarks or schizons, while completely nontrivial, do not have the compelling necessity of Yukawa's arguments, even as they were put forward and a fortiori today. Yukawa had a few rough but clear experimental findings. He also had what now appears to be some very solid general arguments; that is why although the journal (*The Proceedings of the Physical and Mathematical Society of Japan*) in which he published was quite obscure, and his conjecture fairly wide, it was both in Japan and abroad, early taken quite seriously. I should say a word about why we in California took it seriously, because we had some advantages as well as obvious disabilities not shared by our colleagues in Europe. To that I shall return.

On predicting particles

At this point it may be instructive to recall a few episodes of the last thirty years, to pay some attention to other mesons that were correctly and thoughtfully anticipated, either on grounds of symmetry (which has been a very important argument since charge symmetry and charge independence, and still more important since the recognition of unitary symmetry by Gell-Mann and Ne'eman) or, on the other hand, on deep and general principles of relativity and quantum theory. These are principles so general that they are quite beyond the disputes between theoretical physicists who like only S-matrices, those who trust only axiomatic field theory, those who like Lagrangian methods or Feynman diagrams. For all of these are intended to be consistent with the special theory of relativity and the general framework of quantum mechanics.

The first enlargement of Yukawa's invention was an application of the symmetry of charge independence: Kemmer's recognition that this required a neutral meson in addition to Yukawa's charged particles. Although strangeness was a discovery anticipated by no one, once it was found it was at the same time clear that there must be strange mesons. The rho meson was predicted on the basis of empirical nucleon form factors, but with the use of quite general arguments of quantum field theory. The eta meson was a clear prediction of unitary symmetry; so too was the omega, also needed to complement the rho in explaining isovector nucleon form factors.

If we try today to think back to 1935, it is a little strange. A great deal that we now take for granted was then quite uncertain or obscure. Yet I think it will be clear why Yukawa's argument, though very fresh, was still so persuasive. For that we had best start with 1932, the beginning of the growth of our knowledge of particles. Until then,



SIX CHARGED PRONGS emerge from p-p interaction in bubble chamber, including four π mesons, two of which undergo μ-e decay.

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only the electron and proton were known, and the quanta of classical fields. In that year Chadwick discovered the neutron. The neutron, as you know, was predicted, not on all the grounds that would be clear in 1932, but on many, by Rutherford in his 1920 Bakerian lecture. Chadwick, who of course knew that lecture well, once asked the Joliot-Curies why they had not looked for the neutron in the penetrating radiation they had discovered, instead of leaving that to Chadwick. They answered that it was unthinkable for Frenchmen to look to a public lecture for any new idea.

That same year saw another discovery that also had much to do with Yukawa's invention: Anderson's discovery of the positron in the cosmic rays. One great change this discovery brought to the physicists of that day: an increased confidence in the general soundness of that combination of quantum theory and relativity that was quantum electrodynamics, and more widely by analogy, quantum field theory. For myself, this confidence was greatly reinforced by the paper of Pauli and Weisskopf, establishing that spinless particles, with no exclusion principle, no "filled" sea, no "holes," nevertheless were involved in the processes of pair formation and annihilation, manifested charge symmetry—charge-conjugation invariance—and, despite initial disparities of language, closely paralleled the theory of the electron and positron and supported the conjecture that all charged particles would have their own antiparticles of the same mass.

Well before Yukawa's paper, Fermi had written his well known review and most welcome simplification of quantum electrodynamics that led him to a description of beta decay in which the emission and absorption of electron and neutrino are treated in analogy with the emission and absorption of light quanta. Here was another field, the electron-neutrino field, and thus another field theory; in Japan, in the Soviet Union, elsewhere in Europe and in this country it was noted that Fermi's theory of beta decay implied the existence of forces between proton and neutron; for the neutron could emit an electron and neutrino, and a proton could absorb them. There would be an exchange of particles, of momentum and of charge. Although charge exchange was an expected feature of these forces, neither the apparent weakness of the beta-induced forces at nuclear distances, nor their dependance on internucleon distance seemed familiar; for the observed forces are strong and characterized —unlike electricity and gravitation—by a fairly sharp range ($\sim 10^{-13}$cm) characteristic of nuclear dimensions.

The range of forces

At this point Yukawa made his theory. He had reported on the electron neutrino theory. Nishina had shown great interest but also saw clearly the inadequacies. Yukawa came up with something better.

Yukawa noticed that the existence of a range of forces could be understood if the quanta of the field of force had a finite mass. Thus he wrote, instead of Poisson's equation

$$\nabla^2 \phi + \left(\frac{\mu c}{\hbar}\right)^2 \phi = \rho$$

where ϕ is the potential of the field, μ the mass of its quanta and ρ the nucleon density, the source of the field of which ϕ is the potential. One then finds, for distance r large compared to the dimensions of ρ

$$\phi \approx \frac{1}{r}\, e^{\,-\mu c r/\hbar}$$

This is a good explanation of range. From it Yukawa estimated the mass be two or three hundred electron masses.

Yukawa embellished this proposal with one other logically quite separable notion: that these mesons, since they did not seem evident in ordinary matter, were radioactively unstable, and that their radioactivity accounted for the beta activity—the Fermi interactions—of nucleons. Today this has been proposed of the schizons, which have no strong interactions; strongly interacting particles are much too closely related for an entirely obvious distinction to indicate the "primarily" radioactive ones.

The inherent generality of Yukawa's argument for the meson was widely appreciated, and first

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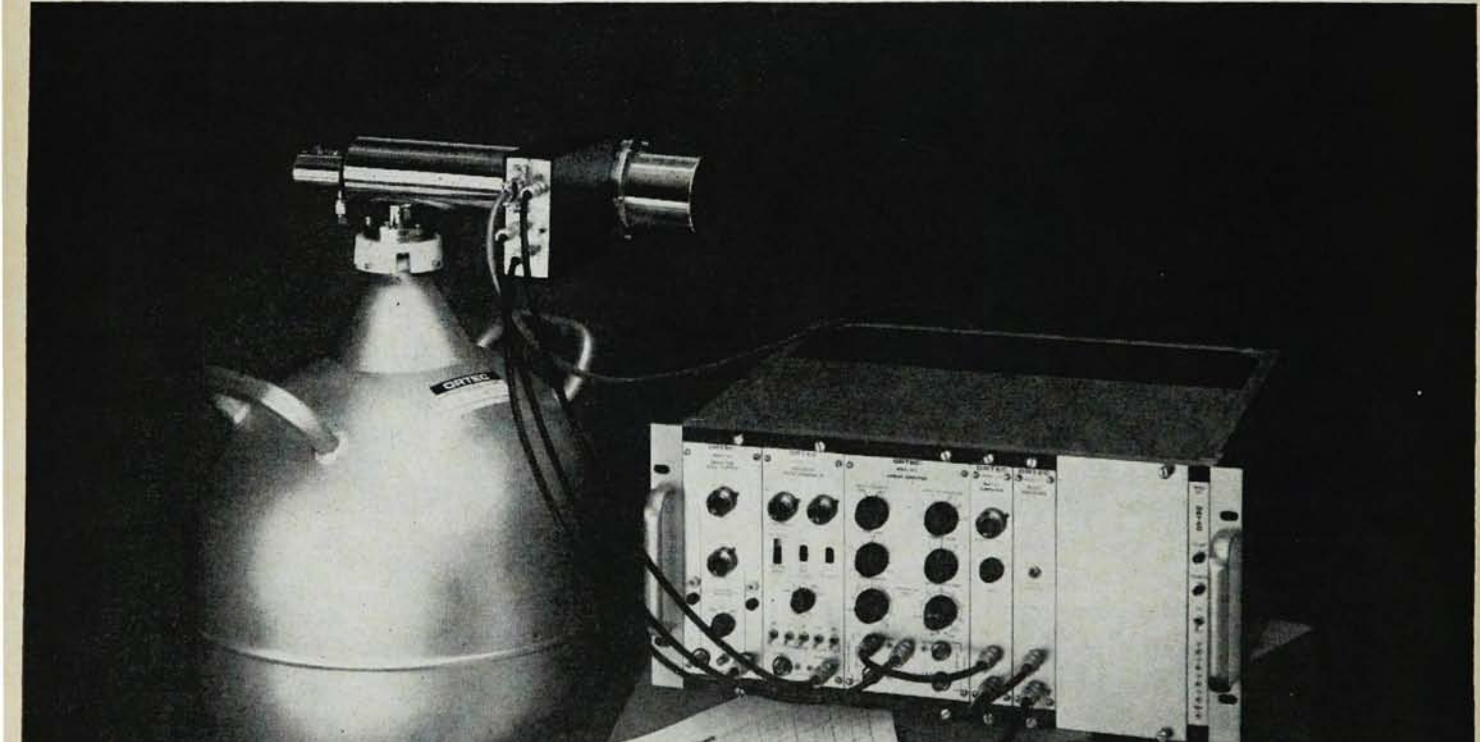


published, I think, by Wick. If at first you treat the meson as relatively light, which is, of course, not necessary but was initially helpful, then when a nucleon emits a meson of mass μ, this costs an energy not less than μc^2. By the uncertainty relations, this state cannot endure much longer than $\hbar/\mu c^2$; and since the meson cannot travel faster than light, it cannot reach much further than $R \approx \hbar/\mu c$: This is the connection between range R and mass, which has become a recurrent and essential argument in physics and is used over and over in many forms, not least in dispersion theory and in approximate treatments of the analytic structure of scattering amplitudes and form factors.

Cascades and cosmic rays

In the midthirties our situation in California was rather special, because Anderson was there, and his work on the cosmic rays, and later that of his collaborators. He used to show us his marvelous cloud-chamber pictures; the more beautiful and marvelous they grew the sadder he became. The origin of the story—perhaps known to you—about mermaids lies here. I took Pauli to Anderson's laboratory, to look at some of his recent pictures. Anderson kept shaking his head and looking melancholy, because the situation they revealed seem so complicated and hard to understand or figure out. Pauli was fascinated, and asked: "What did you expect to see, that you are so sad—mermaids?" The words have been repeated in quite other contexts.

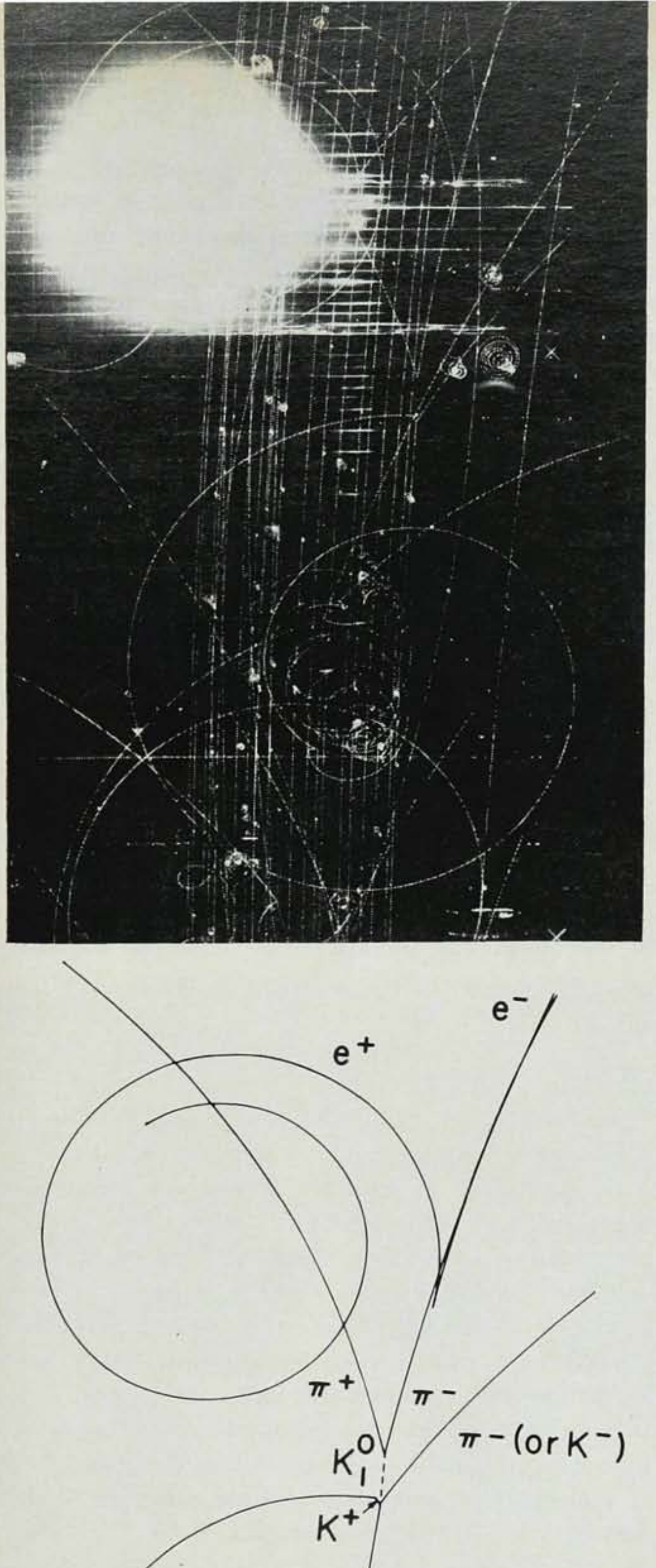
Our confidence in the soundness of the foundations of field theory was one reason why we thought that Yukawa's mesons should exist; another was in the end quite persuasive to me. After 1932, it was recognized that the principal absorption of gamma rays in matter would be, not the Compton effect, but pair production, with a large and constant cross section for high energy, from pair production in the nuclear electric field. A similar formula and asymptotic energy independence described the gamma radiation of an electron or positron passing through this field. These predictions made it awkward to identify any penetrating radiation, charged or neutral, with electrons, positrons or gamma rays. Anderson's pictures showed countless trajectories penetrating substantial plates of lead and gold without absorption or detectable interaction. Indeed at sea level this is true for more than three fourths of the cosmic rays. Of these about half, though not quite, were negatively charged. The only known negatively charged particle was the electron.

By '35 or '36 we had persuaded ourselves, as others had earlier, that the quantum electrodynamic formulas for gamma radiation and pair production had little chance of being wrong for cosmic rays, on the convincing ground that these phenomena involved transfers of four-momentum that are not large, and quite in the range where the formulas have been well checked; further, the electromagnetic fields involved were not unusually strong. Thus, the phenomena were the Lorentz transforms—because of the cosmic rays' high energy—of situations already rather well explored in the laboratory. We therefore concluded that electrons, gamma rays, positrons would all have poor penetrating power, being rapdily converted and degraded by a series of radiative collisions and pair productions, in what we then called "multiplicative showers," what were soon after and much better called "cascades." Of this Carlson and I made a theory, which of course described the Rossi transition curves and the great air showers of Auger. That seemed to settle the behavior of these components. What were the rest of the cosmic-ray particles, the majority, positive, negative and comparatively vastly more penetrating?

Mesons in cosmic rays

It was Anderson's opinion that they could not all —these penetrating particles—or even predominantly be as heavy as protons. They were thus unidentified and unknown. Thus encouraged, Serber and I got ready a short note, arguing that Yukawa's mesons did exist, and that here, in the cosmic rays, they were. After Anderson himself and Neddermeyer published their results and Street and Stevenson found about the same things, we published our note; that was followed in a month or so by a closely related paper of Yukawa. We did not, as we originally intended, include evidence of the radioactivity of the mesons, for Bohr persuaded us that that was a logically quite separable point, as indeed it was; but within a year Blackett published that, with better evidence than we had found.

It is natural that in Europe the response to Yukawa's paper should have been a little slower; it seems not to have been noted until the connection with cosmic rays had been published. There is now a very interesting account of the developments, prepared by Kemmer for the 30th anniversary celebrations of the meson in Kyoto last year. Kemmer himself played an important part in the story. At first, of course, one looked in Japan, in Europe and in this country at



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BUBBLE-CHAMBER PHOTOGRAPH of π^--p interaction, producing a K^+, a K_1° (shown by dotted line), a π^-, and a neutron (no track). The K^+ quickly decays into π^+ and π° (no track). The π^+ decays into a μ^+, which decays into an e^+. The π° decays into a pair of gammas (no tracks), one of which creates an e^--e^+ pair. K_1° decays into π^+ and π^-.

what might be the spin and parity of the mesons; scalar mesons, as originally proposed, hardly seemed promising for nuclear forces. We could show, of the cosmic-ray mesons, that they could not have spin one, because then they too would radiate gamma rays too much to be as penetrating as observed. We thought that they were charged pseudoscalar fields. We noticed, with growing but much too well contained alarm, that they showed very little nuclear interaction: they showed no appreciable nuclear scattering besides multiple Coulomb scattering; the limits, still not very sharp, were ominous. We tried to explain this by developing strong coupling theory, which can indeed give very much reduced nucleon scattering.

All of this of course later turned out to be nonsense. No one had ever predicted *these* cosmic-ray mesons. No one then knew what they were; no one understands their existence today; no one expected it; no one has a good argument as to why they should exist, nor have the properties they have.

It was in Europe that Kemmer looked at another question: not the spin and parity-transformation properties of the fields but their charge-bearing properties and thus the charge dependence of the resulting nuclear forces. After the careful work of Breit, it was hard to doubt that the forces between proton and neutron had much in common with those between proton and proton. With charged mesons only, this would be most puzzling since the exchange of a single charged particle, which gives exchange character to proton-neutron forces, is impossible between proton and proton. Kemmer, therefore, not only postulated neutral mesons, but devised a charge-independent, charge-symmetric as well as charge-conjugation-invariant theory of the interaction of mesons and nucleons, a theory not only charge symmetric but leading to exchange forces. It was soon widely observed that these neutral mesons, charge self-conjugate, should be unstable for gamma decay, whether into two or three quanta depending on the quantum numbers of the meson describing its space-time symmetries.

Interruption

At this point, many of us, in one way or another, went off to war. We had the impression that the strong meson-nucleon coupling presented problems that were theoretically very tough, and probably beyond us. We did not understand the small cosmic-ray meson-nucleon scattering. We thought Yukawa's general ideas right and that one looked at his mesons in the cosmic rays at sea level. In some countries, among our Japanese colleagues, in England, in Italy, valuable work continued; at the Princeton Institute Pauli and his colleagues worked out several forms of strong-coupling theory. The development which was later to make the decisive clarification derived from the experiments of Conversi et al in Rome. They showed that the interaction of cosmic-ray mesons with nuclei was extraordinarily feeble, of the order, as was later recognized, to be expected of Fermi beta-decay interactions, not of the order of the very strong nuclear forces, or fields that could produce them. Indeed the observations compared the radioactive decay rates of negative mesons with their capture from mesic stationary atomic states; for light nuclei the rates are comparable. When word of all this became known, it was far more decisive than the small nuclear scattering in proving that cosmic-ray mesons did not do what Yukawa invented mesons to do: react with nucleons and thus mediate nuclear forces. Fermi and Teller analyzed the mesic atoms and showed that there was an overwhelming chance that mesons so bound would be captured before decay if the Yukawa process were of expected or reasonable probability. In June of 1947, at the first serious and intimate conference after the war, these problems were discussed, as were the Lamb shift, and the electron's anomalous magnetic moment, and renormalization in quantum electrodynamics. Then Bethe and Marshak proposed that the particles one saw in the cosmic rays might be relatively inert decay products of Yukawa's mesons. This was indeed found by Powell and Occhialini with their new Ilford plates. It was clear then that for ten years we had misread the particles.

Strong interactions and strangeness

With the discovery of the pi meson, and the reconfirmation by direct neutron-proton scattering experiments of the exchange character of nuclear forces, it became natural to think again of the neutral meson. We recognized that it would give rise to a copious production of the soft component of cosmic rays, because of the gamma decay; this would explain the Auger showers, without any appreciable number of primary electrons or gamma rays. Then the neutral pi meson was also found in Berkeley. It decayed into two gamma rays, in harmony with the view that the Yukawa pi-meson field was a pseudoscalar field.

One then had in earnest the problem of how to describe meson-nucleon interactions. For years one had tried perturbation theory in analogy with electrodynamics; but even before the war the strength of the coupling made that seem, as it was, a most unpromising procedure. Nor were the postwar efforts with the related Tamm-Dancoff treatments more fruitful in their results, as they were no more secure in their foundation. Thus in some ways the strong-coupling approximation, which rested at least initially on treating the meson mass as very small compared to that of the nucleon, gave more relevant insight: It predicted a low-lying isobar of spin 3/2 and isospin 3/2, as an example. When Fermi and his laboratory began to study the scattering of Yukawa mesons—and not the inert mesons of cosmic radiation—on nucleons, this isobar appeared as a very prominent feature, indeed a resonance, in the scattering amplitude. Just this gave impetus to the development of more adequate and very slightly more sophisticated ways of dealing with the strong interactions. The first work of Chew (similar in approach to strong coupling theory) and then Low's equation, crossing, and forward-dispersion relations (rediscovered by Goldberger and greatly extended by him and many others) led to a framework within which—with dispersion relations and the analyticity they express, with crossing symmetry, with unitarity—one could again describe the properties of strongly interacting particles. As of today, this machinery in general provides very meager powers of accurate a priori prediction but does provide a description in which empirical knowledge and conjectured theoretical regularities can be expressed and exploited. Jost has characterized the earlier unenlightened dependence on perturbation theory as requiring a moderately adequate familiarity with the Latin and Greek alphabets, and none whatever with mathematics.

The last great discovery that cosmic rays contributed to particle physics was the discovery of strange particles. Here the obvious paradox lay in the abundance of their production from nonstrange matter, compared to their slow decay back to such matter. It was clear that here the pi-mu meson trick would not work; but when one strange particle was produced from nonstrange matter, another of opposite strangeness would appear with it; this probable process would lead to two particles which of themselves could only by weak forces —and slowly—decay into nonstrange matter because for each the decay involves a change in strangeness and thus no fast reaction. It thus was clear that if a negative pi-meson—proton collision produced a lambda particle—which would, two times in three, give back a proton and a negative meson—a strange meson of strangeness opposite to the lambda must appear in the primary collision.

The story of the strange mesons was itself quite a story, centering on the relation of the tau and theta mesons, which decayed into three and two pi mesons respectively. I shall not remind you of what a tangle and trouble that was, until the two mesons were recognized as one, and the lack of parity conservation in weak interactions, suggested by Lee and Yang, was established by Miss Wu and her collaborators. With all this we are surely not finished, not nearly. We do not understand the rare decay of the long lived K meson into two pi mesons, though there are many speculations, including some, largely from Lee, that would replace this mystery with a larger and deeper one, of a mismatch between charge and baryon-number conjugation.

Puzzles that remain

With the K and pi mesons, one had seven, all described by pseudoscalar fields. A quite different source of enrichment was the analysis of the electromagnetic form factors of the proton and neutron. In perturbation-theoretic terms, one expected the proton to be a well localized charge, replaced by a more diffuse one when the proton was dissociated into neutron and meson. This picture did not find any support empirically, and for a time it was not clear what physical reactions determined the charge distributions. Writing down the dispersion integral for the form factor, one saw that arguments of relativity and complementarity had once again something to say: Frazer and Fulco showed that there must be, to understand the form factors, at least a pi-pi complex —an object of spin 1 and isospin 1, bound though not stable—and estimated where its mass should lie and how unstable it should be. These mesons were looked for and in time found. Theoretically, I fear, no clear calculation of their properties, their mass and width, has been possible so far, and surely not for want of trying. It also is clear that another vector meson, this one an isoscalar (largely indeed an omega) is needed for the form factors. Needless to say, we are not finished with the form factors either, especially at high momentum transfers.

With the discovery of the K resonance, the four spin 1, isospin 1/2 excited K mesons, one then had eight vector mesons. An eighth pseudoscalar meson had long been conjectured and sought and was found in the narrow, long-lived eta. But this, and the finding of a ninth isoscalar pseudoscalar and isoscalar vector meson, followed the increased confidence of Gell-Mann and other colleagues, that there was a good measure of truth in unitary symmetry and the eightfold way.

Unitary symmetry was surely not the only—or the first—to be tried. It has worked better than others, much better, better than O_4 or G_2 for instance. As for larger groups, their situation was well reviewed in a session of this meeting, to which I refer for an account of the recent work and views, among others, of Dashen and Gell-Mann, Gürsey and Radicati, Adler, Weissberger, Fubini, Cabbibo. These lead me to expect more baryons and mesons to be identified, indications of many of which appear in current studies. Whether we should also expect far more massive, less familiar ingredients, quarks, that should quite directly manifest the order of subnuclear matter, is to me profoundly more doubtful.

Always before in our history, in chemistry, in atomic physics, and nuclear physics, it has been possible to order a large and complex array of material particles as composites of a very much smaller number of more elementary particles interacting according to moderately simple—or approximately simple—laws. Today one has to remember that the quarks would, like baryons and mesons, have to be strongly interacting particles, composites of one another, composites too of their composites. Thus it seems to me unlikely enough that we are about to return to a simple view of the fundamental particles as "simply" composed, as is the hydrogen atom or the deuteron, of simpler things and by simple laws.

It seems to me that we are in for a far greater novelty than the discovery of "more fundamental" particles. It is not one of the privileges, as it is assuredly not one of the virtues, of senility to make predictions. I make only one. I think that we are unlikely to live again through such a ten-year joke as mistaking the mu mesons for Yukawa's particles. I do not think that could have happened if it had not been for World War II. That too, I hope, is not so likely to recur. □